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ABSTRACT

This paper analyzes the effects of the 1988-1992 macroeconomic crisis in Peru on the schooling and employment decisions taken by school-aged children in urban areas. It discusses the Peruvian setting during this period and describes the data used in the analysis and the econometric specification. Two basic findings were made: (1) there was no effect of the crisis on attendance rates of school-aged children, however, the fraction of children who were both employed and in school decreased significantly during the crisis; and (2) mean educational attainment increased significantly for children exposed to the crisis than for those unexposed. The paper argues that these findings may be related. Children who are not employed have more time available and, therefore, may put more effort into school. The analysis concludes with a discussion of the implications of these findings for the design of appropriate social protection policies. (Includes 21 notes, six figures, and four tables. Contains a 41-item bibliography.) (Author/BT)

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The (Positive) Effect of Macroeconomic Crises on the Schooling and Employment Decisions of Children in a Middle-Income Country[1]

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SO 034 000

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Abstract

The impact of macroeconomic crises on the investments made by parents in the human capital of their children is a widely contested topic. I analyze the effects of the profound 1988-1992 macroeconomic crisis in Peru on the schooling and employment decisions taken by school-aged children in urban areas. The basic findings of the paper are two. First, there is no effect of the crisis on the attendance rates of school-aged children. However, the fraction of children who are both employed and in school went *down* significantly during the crisis. Second, mean educational attainment is significantly *higher* for children exposed to the crisis than for the unexposed. I argue that these findings may be related: Children who are not employed have more time available, and may therefore put more effort into school. The paper concludes with some discussion of the implications of these findings for the design of appropriate social protection policies.

1. Introduction

Sudden economic downturns, resulting from factors such as fiscal weakness, weak financial systems, changes in prices of primary commodities, and natural disasters are a reality in many Less Developed Countries (LDCs), and will continue to be a concern in the future. Countries in East Asia faced a massive economic contraction in 1998; Latin American countries generally had low or negative growth rates during the "lost decade" of the 1980s, and many countries in the region, including Argentina, Brazil, Ecuador, Mexico, and Peru faced sharp, precipitous drops in GDP; very steep economic downturns are also an unfortunate fact of life for many African countries.

There has been a recent surge in academic and policy interest in the effect of macroeconomic crises on household behavior. Of particular concern are household investments in human capital. An oft-made argument is that households who are unable to smooth consumption perfectly may cut back the expenditures they make on the education, health, and nutrition of their children during a crisis. The World Bank's flagship publication, World Development Report 2000/01: Attacking Poverty, strikes a particularly dire note: "When a crisis strikes and households cannot borrow or when adult unemployment is high or wages low, children are pulled out of school and sent to work. The lost schooling leads to a lifelong loss in earning ability for these children. Failures in the credit or labor markets thus transmit poverty and vulnerability across generations" (World Bank 2001, p. 146; see also Lustig 2000).

The theory of human capital suggests that macroeconomic crises can affect the total amount of schooling, the timing of this schooling, and the effort which is devoted to schoolwork. In the standard neoclassical model of human capital investment, individuals acquire schooling until the (expected) marginal benefit to an additional year of education equals the marginal cost (Rosen 1977; Willis 1986).

The marginal benefit is given by the increase in the discounted expected stream of earnings resulting from one more year of schooling. The marginal cost of an additional year of schooling is given by the foregone income, and by direct private costs such as tuition and transportation. The marginal cost will be increasing in years of schooling (or age) if net forgone earnings or tuition increase with schooling. Borrowing constraints may be an additional cost of attending school because they prevent consumption smoothing (Becker 1964; also Jacoby 1994 for an analysis of Peru). Low-income households, in particular, are more likely to both be unable to borrow and to lack "internal finance" for education purposes.

What is the effect of an adverse macroeconomic shock on the schooling decision? In general, a crisis will depress current employment and wage prospects, and the opportunity cost of

attending school represented will fall. Holding everything else equal, this should lead to an increase in human capital investment. But a macroeconomic shock could also make borrowing constraints more binding, and thus depress the total amount of schooling chosen.

When macroeconomic shocks are persistent, they may also depress expected lifetime earnings, and have an effect on the marginal benefit from schooling. If the lifetime earnings of all individuals are reduced by the same percentage, regardless of their schooling, then the marginal benefit associated with an additional year of schooling will be lower (by this same percentage). But crises need not have such a uniform across-the-board effect on expected earnings. For example, if the expected lifetime earnings for individuals with less education are disproportionately affected by the crisis, the marginal benefit to schooling could rise.

Insofar as they change the marginal costs of schooling, macroeconomic crises may also affect the timing and intensity of schooling—in particular, the extent to which children combine schooling with part-time employment. Finally, the effect of a crisis on the wages and employment prospects of *adults* in a household may also have an effect on the schooling and employment decisions taken by *children*.

The basic point of this discussion is that the effect of a macroeconomic crisis on schooling is indeterminate in theory. Children (or their parents) may seek more or less total schooling, they may anticipate or postpone further schooling, and they may expend more or less effort in school (for example, by combining school and work). The total effect of a crisis on schooling will depend on the relative magnitude of the changes in the marginal costs and benefits from education, as well as on the cross-price elasticity of child employment and adult wages.

Not surprisingly, the empirical evidence on the effect of systemic shocks to household income on schooling outcomes is mixed. In the United States, Goldin (1999) finds a large increase in secondary school enrollment rates during the Great Depression, especially in those states that were hardest hit by unemployment. In the LDC setting, Neri and Thomas (2000) suggest that enrollment decisions are unaffected by macroeconomic conditions in Brazil, although grade attainment appears to suffer during a crisis. Cunningham and Maloney (2000) find that girls (but not boys) are more likely to drop out of school during a crisis in Argentina, while both boys and girls are *less* likely to drop out during crises in Mexico.[2] Using cross-country regressions, Flug, Spilimbergo and Wachtenstein (1998) report that macroeconomic shocks have negative effects on enrollment, and Behrman, Duryea, and Szekely (2000) suggest that the poor macroeconomic prospects of the 1980's in Latin America set back the rate of growth of schooling attainment in the region. In Indonesia, Beegle, Frankenberg and Thomas (1999), Cameron

(2000), Filmer et. al. (2001) and Pradhan and Sparrow (2000) all report some impact of the crisis on enrollment, although the effects tend to be small.[3]

This paper analyzes the effect of the prolonged economic crisis in Peru between 1988 and 1992 on school attendance, employment, and the number of years of completed schooling. The Peruvian case is appealing from an analytical point of view for a number of reasons. First, the crisis in Peru was particularly deep and prolonged: National accounts data suggest that GDP per capita fell by almost 30 percentage points between 1988 and 1992. The depth and duration of the crisis could severely stretch the ability of households to protect investments in schooling. Second, reliable household data are available for Peru. Three Living Standards Measurement Surveys (LSMS) cover the period before the crisis (1985/86), during the crisis (1991), and after the crisis (1997). These LSMSs include detailed information on household characteristics, including demographic structure, income, consumption, access to credit, and parental education, and child outcomes, including educational attainment and employment. The length of the questionnaires and therefore the potential list of controls is much richer than that found in the labor force surveys which have typically been used for analyses of crisis impact (for example, in Brazil and Mexico), although the sample sizes are also smaller. Moreover, it is rare to have high-quality data for a middle-income country which covers the periods before, during, and after a crisis.

The rest of the paper proceeds as follows. Section 2 briefly discusses the Peruvian setting during this period. Section 3 describes the data used for the analysis; more details on the data can be found in a Data Appendix. In section 4, I describe the econometric specification, and present results. Section 5 concludes.

2. The Peruvian setting

Economic developments, 1985-1997: Peru followed an erratic economic course in the 1980's and 90's. Between 1985 and 1990, the government of Alan Garcia attempted to stimulate the economy with a "heterodox" stabilization program, which relied, inter alia, on reduced foreign debt payments, a price freeze, and economic reactivation via wage increases, job creation programs, and increased investments in education and health. These policies encouraged high growth rates in 1986 and 87, but the inflationary pressures and budget deficits quickly proved

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[2] A summary of the evidence for Latin America can also be found in de Ferranti et. al. (2000, pp. 73-87).
[3] Note that it is important to distinguish between the effect of *systemic* crises, such as macroeconomic contractions, and *idiosyncratic* crises, such as loss of employment or illness of a household breadwinner on schooling decisions. Systemic crises are generally associated with worse employment prospects for children, whereas idiosyncratic crises need not be. As a result, household responses may differ.

unsustainable (Glewwe and Hall 1994). As a result, the country slid into a deep recession and hyper-inflation in 1988. By the end of Garcia's government in 1990, the economy was in a state of near-collapse: GDP per capita had fallen by -10.5%, -13.4%, and -6.9%, and inflation had reached an incredible 667%, 3399%, and 7482%, in 1988, 89, and 90, respectively (see Figure 1). The economic crisis came hand in hand with a deterioration in public safety: By 1990, there was a state of virtual civil war between the armed forces and the insurgencies of Shining Path and Tupac Amaru Revolutionary Movement (MRTA) in some areas of the country, and the conflict was claiming at least 2,000 lives per year (INEI 1996, p. 58).[4]

The Fujimori government which took office in 1990 opted for more orthodox economic remedies. Economic reforms included the elimination of controls on prices, interest rates, and foreign exchange transactions, the reduction of tariffs, labor market de-regulation, and a far-reaching program of privatization. The new policies had an immediate effect on inflation, which fell to 74% in 1992, and to less than 12% by 1995. Growth picked up after 1992 (Figure 1), and poverty decreased between 1991 and 1994, and again between 1994 and 1997. Like the crisis, the recovery appears to have been far-reaching, affecting all regions and most households (World Bank 1995 and 1999a).[5] Meanwhile, order was gradually re-established in the countryside, in particular after the capture of Abimael Guzman, Shining Path's leader, in 1992.

Education in Peru, 1985-1997: In comparison with many of its Latin American neighbors, and with other countries with similar income levels, Peru has managed to attain impressively high enrolment rates in both primary and secondary school: Analysis of the 1997 LSMS suggests that 97.5% of 6 to 11 year-olds, 86.3% of 12 to 17 year-olds, and 34.9% of 18 to 25 year-olds were attending some educational institution.[6] These figures are particularly impressive because of the low levels of public spending on education: In 1997, total public spending on education in Peru was 3% of GDP (2.4%, net of spending on pensions), compared with the Latin American average of 4.5% (World Bank 1999b).

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[4] These figures are based on incidents reported by the Ministry of Interior, but are likely to be under-estimates. Other estimates place the number of deaths from terrorism in 1990 above 3,000 (see, for example, World Bank 1995).

[5] Strict comparisons in poverty measures are not possible because of differences in the coverage of the surveys (see section 3).

[6] Author's calculations, based on the 1997 LSMS tapes. Note that these numbers do not correspond to those in Table 1 for two reasons. First, these are numbers for all seven regions in Peru, including rural areas and the urban jungle, whereas the numbers in Tables 1a and 1b correspond to Lima and the urban areas of the coast and sierra only. Second, these are means weighted by the appropriate (individual) expansion factors, while the figures in Table 1 are unweighted means. Figures for school attendance for 6 to 14 year-olds based on the 1996 Peru Demographic and Health Survey (DHS) can also be found in Filmer (1999). These tend to be quite similar, except for 6 year-olds, which appear to have much lower enrollment

Private expenditures on education in Peru are about 2% of GDP, well above the OECD average of 1.3%. As in other countries, these expenditures vary across education levels and income quintiles. In 1997, households spent about 3 times as much to send an 18 to 25 year-old to school as they did to send a 6 to 11 year-old. (Most of the older individuals would be attending university or some other tertiary institution, while the younger children would be in primary school.) Differences across income quintiles are starker: On average, households in the richest income quintile spent about 8 times as much for every child aged 6 to 11 attending school as did households in the poorest income quintile, 5 times as much for those aged 12 to 17, and 4 times as much for those aged 18 to 25. Still, by international standards, these differences in the expenditure patterns across income quintiles are modest.[7]

In Peru, education at the primary and secondary levels is dominated by the public sector: Ministry of Education figures suggest that 88.2% and 84.0% of students in primary and secondary school, respectively, attend a public school; these percentages have been virtually unchanged since 1990, when they were 88.3% and 85.6%. The private sector plays a much larger—and growing—role at the tertiary level: 43% of all tertiary education students (university and non-university) were enrolled in a private institution in 1997, up from 37.6% in 1990 (World Bank 1999b, Volume 2, p. 53).[8]

Public provision of education has traditionally been quite centralized in Peru. In 1991, however, the government initiated a policy of deconcentration to twelve regional governments. As a result, a growing fraction of resources has been transferred directly to the regions: In 1990, the Ministry of Education managed 71% of total public expenditures on education, and the regions 17%, but by 1997 these figures had been reversed, with the ministry of Education managing 25%, and the regions 56% (World Bank 1999b, Vol. 1, p. 16).[9] The regional governments which manage education do not have capacity to raise own revenues, and are appointed by the central government rather than elected.[10]

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rates in the DHS (61.3%), likely as a result of differences in the wording of the question or in the timing of the survey during the school year.

[7] As a comparison, in the Philippines households in the richest quintile spend 20 times as much per student in primary school as households in the poorest quintile, 11 times as much per student in secondary school, and 11 times as much per student at the tertiary level (Schady 2001, p. 34).

[8] See the caveat given in the Data Appendix on the reliability of enrollment and repetition statistics provided by the Ministry of Education.

[9] Autonomous universities and other government bodies manage the remaining 19%.

[10] Municipal governments at the province and district levels, by contrast, are elected.

3. Data and sample means

Data: The main sources of data used for this paper are the 1985/86, 1991, and 1997 Peru Living Standards Measurement Surveys (LSMS). These surveys contain reliable information on household characteristics, school attendance, grade attainment and labor force participation.

The questionnaires in all three surveys are generally comparable.[11] Unfortunately, the coverage of the surveys is not comparable across years. While the 1985/86 and 1997 LSMS visited households in each one of Peru's seven "natural regions"—Lima, and the urban and rural areas of the coast, sierra (or highlands) and selva (or jungle)—the 1991 LSMS sampled households only in Lima, the urban coast, and the urban and rural areas of the sierra. There is also some reason to be concerned about the 1991 sample for the rural sierra: Mean income in the rural sierra appears to be *higher* in 1991 than in 1997, and the mean years of education of parents in the rural sierra approximately doubles between 1985/86 and 1991, and stays constant thereafter (see Table 1a below). It is likely that lack of funding for the 1991 survey, or the dangerous conditions caused by widespread terrorism in the rural sierra in the early 1990's prevented enumerators from visiting outlying, poorer, less well-educated households in this region in 1991. I therefore limit the sample to Lima and the urban areas of the coast and sierra for all of the analysis. In 1997, these three regions jointly accounted for approximately 58.1% of the population of the country. (Separately, I also run all of the regressions I report in the paper in samples which include the rural sierra. In all of these results, which are available from the author upon request, the crisis impact was larger in absolute value than those I report for the urban sample.) More details on other sources of data and their reliability, as well as on the construction of the variables used for my analysis can be found in a Data Appendix.

Sample means: Tables 1a and 1b suggest that the 1988-1992 crisis affected all regions, and all income quintiles. Income dropped throughout, and credit became harder to come by: The fraction of households who report having had access to credit is lower in 1991 (16%) than in either 1985/86 (22%) or 1997 (40%). Tables 1a and 1b also show predictable secular trends in parental education (rising), age (rising), and household size (falling).

Children (or their parents) make decisions about schooling and employment jointly. In so doing, they sort themselves into one of four mutually exclusive categories: Those who attend school only, are employed only, are neither attending school nor employed, and are both attending school and employed. In Tables 1a and 1b, I show unconditional, unweighted means

[11] There are some unfortunate exceptions. For example, there are differences across surveys in the recall periods and in the level of disaggregation that is asked in the questions concerning household expenditures on education.

for these four categories, the fraction of children who have completed a standard number of years of schooling by age, and a number of variables which are likely to have an effect on schooling and employment decisions, by survey year. Table 1a presents results for the "full" urban sample (excluding the urban selva, which was not included in the 1991 survey), and for each strata separately. Table 1b breaks down the full urban sample into men and women, and compares the first (poorest) and fifth (richest) income quintiles.

I present results on attendance and employment patterns separately for children aged 6 to 11, roughly corresponding to primary school, and children aged 12 to 17, roughly corresponding to secondary school. Tables 1a and 1b, and Figure 2 show that attendance levels for school-aged children in urban areas are very high throughout: For children aged 6 to 11, the attendance rate is always 97% or higher, while for children aged 12 to 17 it is always 90% or higher. By and large, children go to school in urban Peru. But the extent to which they combine school with work changes dramatically across years. Figure 3 shows that the fraction of children who are working is lower at every age in 1991 than in 1985/86 or 1997. Differences are particularly large for the older children. In the full urban sample, 31% and 21% of children aged 12 to 17 are both attending school and employed in 1985/86 and 1997, respectively. In 1991, by contrast, only 12% of the children in this age group combine school and work. This pattern holds consistently across strata, for girls as well as boys, and for children in the richest and poorest income quintiles.

The 1988-92 crisis is too recent to evaluate its effect on the total amount of schooling chosen by children. However, the data can be used to assess the extent to which children of a certain age have completed a given number of years of schooling. I do this in two ways. First, I calculate the fraction of children who have completed an appropriate number of years of schooling for their age, by survey year. Children in Peru are expected to start first grade at age 6 or 7. Therefore, unless they start school late or repeat a grade, they should have completed first grade by age 8, second grade by age 9, and so on. I consider children to be "on-track" if they have completed *at least* this number of school years for their age. Note that this is a generous definition of on-track, as children who start school at age 6 should have completed first grade by age 7.

The second approach I take pools data from the three surveys, and calculates the mean years of schooling completed by children in different birth cohorts. All school-aged children surveyed in 1985/86 were unexposed to the crisis, while all school-aged children surveyed in 1991 were exposed to the crisis. By contrast, there is a discontinuity in exposure for school-aged children surveyed in 1997: Those aged 11 through 17 were exposed to the crisis during their school-aged years, while those aged 6 through 10 were not.

To compare differences in the fraction of children who are on-track across years, I limit the sample to 8 to 10 year-olds. This is because 6 to 7 year-olds are on-track by construction, while children aged 11 and older in 1997 were exposed to the crisis during their school-aged years. Tables 1a and 1b show that, conditional on attendance at school, the fraction of 8 to 10 year-olds who have completed the minimum appropriate number of school years for their age is higher in 1991 than in either 1985/86 or 1997. In the whole sample, 87% of 8 to 10 year-olds were on-track in 1991, but only 72% and 79% in 1985/86 and 1997, respectively. Here too this difference across years holds for all regions, for girls and boys, and for the poorest and richest income quintiles. Note that these comparisons could suffer from selection bias because I condition on attendance—in particular, if "marginal" students who are less likely to make satisfactory progress in school are more likely to start school late or drop out early. I do not think that this is an important source of concern, however, given that there are only very small variations in attendance rates across years.

Figure 4 graphs the mean years of schooling completed by age for children exposed and unexposed to the crisis, without conditioning on attendance. The graph also includes a 45 degree line which corresponds to on-track school progression. The results in Figure 4 are very similar to the findings for the probability of being on-track in Tables 1a and 1b. Mean years of schooling are below the ideal on-track progression, especially at higher ages, because of school desertion and the cumulative effects of late school entry and repetition. However, children exposed to the crisis have an average of between .1 and .2 more years of schooling. There is no clear pattern whereby the unexposed children fall further behind or catch up with age.

4. Econometric specification and results

I next pool the data from the three surveys, and turn to multivariate regressions to estimate differences in school attendance, employment, and school attainment. First, I estimate logit regressions with a dependent variable which takes on the value of one if children are both attending school and working, and zero if they are attending school only. These regressions include dummy variables for the two "non-crisis" years (1985/86 and 1997), as well as other controls. The omitted category is therefore the "crisis" year of 1991. I report the odds-ratios for the 1985/86 and 1997 dummies, and interpret these as year effects conditional on changes over time in household characteristics. Pervasive, predictable differences across years could be consistent with a causal effect of the 1988-92 crisis on attendance and employment. Second, I estimate OLS regressions in which the dependent variable is the number of years of schooling completed. These regressions include dummy variables for two survey years, as well as a

measure of the number of years that school-aged children were exposed to the crisis. The range of this exposure variable is therefore between zero (children who were never of school-age during the crisis) and five (children who were of school age during every year between 1988 and 1992). (Details of these exposure calculations can be found in the Data Appendix.) I interpret the coefficients on the measure of exposure as the marginal effect of a year of exposure to the crisis on the years of schooling completed.

In both instances, I report results from a number of specifications. In addition to the survey year dummies (in both the logit and OLS regressions), and the measure of crisis exposure (in the OLS regressions), specification (i) includes strata dummies, gender dummies, and unrestricted age dummies. Information on the years of completed education of both parents (separately) is also available for a sub-sample of children—specifically, those who live in households in which both parents are present and whose relationship to the household head is son or daughter. Specification (ii) therefore adds controls for the education of both parents (separately), unrestricted dummies for the age structure within the household, and a main effect in household size. The LSMS tapes include identifiers with which to map households in the surveys to the districts in which they live.[12] The fixed-effects specification in (iii) limits the analysis to households living in districts which were included in the sample in 1985/86, 1991, and 1997, and includes the variables in specification (ii), as well as a dummy variable for each district. These regressions therefore identify differences in attendance levels, employment levels, and educational attainment from *within-district* variation across survey years or birth cohorts. (Note that district fixed effects are *not* a satisfactory solution for the problem of differences in the sample in the rural sierra between the 1985/86 and 1997 surveys on the one hand, and the 1991 survey on the other. The likely problem with the 1991 sample for the rural sierra is that enumerators may not have visited outlying areas *within* a given district, rather than not visiting outlying districts.) One disadvantage of the estimations which include measures of parental education and those with district fixed effects is that both are limited to sub-samples of the data. In the three regions considered in the analysis in this paper, we only have data on the schooling of both parents for 73.9% of 6 to 11 year-olds, and only 77.8% of 6 to 11 year-olds live in districts which were included in the sample in 1985/86, 1991, and 1997. Better controls therefore come at the cost of smaller sample sizes and less precision.

[12] There are more than 1800 districts in Peru, and these are the smallest political and administrative division in the country (roughly comparable to counties in the United States). According to the 1993 population census, the average district population was about 12,600 inhabitants. Some, predominantly rural districts have less than 200 inhabitants, whereas a handful of urban districts in Lima have more than 100,000 inhabitants. See Schady 2000, p. 3.

Table 2 presents the odds ratios of attending school and being employed, relative to attending school only, for children aged 6 to 11 (upper panel) and children aged 12 to 17 (lower panel). The results show that the odds that a child aged 6 to 11 is both attending school and working, relative to the odds that she is attending school only, are two to four times as high in 1985/86 and 1997 as they are in 1991. These results are significant at the 5 percent level or better in all cases but one (the odds ratio for the 1997 dummy in specification (iii), which has a p-value of 0.07). Similarly, the odds that a child aged 12 to 17 is both attending school and working, relative to the odds that she is attending school only, are two to three times as high in 1985/86 and 1997 as they are in 1991. Once again, these results are quite consistent across specifications, and are always significant at the 5 percent level or better. (Separately, I also estimate multinomial logit regressions which compare the odds ratios of neither attending school nor working, working only, and both attending school and working with the base category of those who attend school only. These estimates, which are not reported but are available from the author upon request, suggest that there are no differences in the odds ratios of those who neither attend school nor work across years, while the odds ratios of working only for 12 to 17 year olds relative to both attending school and working are significantly higher in 1997 than in 1991).

Poorer households are more likely to be credit-constrained than their better-off counterparts. Although the means in Table 1b show no clear differences in outcomes between the first and fifth income quintiles, it is conceivable that the regression coefficients I report in Table 2 do not hold for all income levels. To investigate this possibility, I predict log per capita household income on the basis of variables for the age, gender, and education of the household head, and household size and composition. I then run non-parametric (lowess) regressions of the probability of being both employed and in school on predicted log income, by year. (I use predicted income because of the potential endogeneity of income in a child employment regression, but the results are very similar with actual income.) The results, which are presented in Figure 5, show that the probability of being both in school and employed is lower in 1991 than in 1985/86 and 1997 across the entire income distribution.[13]

In additional (unreported) specifications, I further test for possible heterogeneity by interacting the year dummies in the logit regressions with (separately) a number of household characteristics, such as the education of the parents, household size, and the strata dummies. The

[13] Comparable graphs for the fraction of children who are attending school only, which are essentially the complement of the graphs in Figure 5 given the very small number of children who are neither in school nor working, and those who are working only, show that the fraction of children who are attending school only is highest in 1991 at all income levels.

coefficients on the interaction terms are never significant, suggesting that household responses to the crisis did not vary significantly.

Table 3 presents OLS regression results for the mean years of completed schooling. I report two sets of specifications: In the first, the measure of exposure is continuous (upper panel), in the second it enters as a set of dummy variables corresponding to individual years of exposure (lower panel). The results for the continuous exposure variable suggest that every year of exposure to the crisis is associated with a .04 to .05 increase in the number of years of schooling attained. That is, children who were of school-age for the entire 1988-92 period would have completed about one-quarter more school years than those who were entirely unexposed. Since completed schooling only changes by whole integers, one way of interpreting this coefficient is that one out of every four or five fully exposed children will have completed one more year of schooling than the comparable unexposed.

The dummy variable specifications in the lower panel of Table 3 provide weak evidence that school attainment does not increase *proportionately* with crisis exposure: A "low" amount of crisis exposure—1 to 2 years—appears to have no effect on school attainment, while "high" crisis exposure—3 to 5 years—increases schooling by about 0.2 years. One possible interpretation of this finding is that children or their parents take time to adjust their schooling and employment decisions to a crisis. Too much should not be made of these more nuanced results, however: F-tests on the additional coefficients fail to reject the null that these dummy variable specifications do not represent a significant improvement in fit over the more parsimonious specifications in which crisis exposure enters the regression linearly.

Discussion: Macroeconomic crises can affect the total amount of schooling chosen, the timing of this schooling, and the extent to which schooling is combined with work. The 1988-92 crisis in Peru appears to have reduced access to credit; if anything, this should have led to a reduction in human capital investment during the crisis. The effect of the crisis on the marginal benefits from education is unclear in theory. For this second channel to be part of the explanation of my finding the crisis would have had to be perceived both as persistent and as disproportionately affecting the earnings and employment prospects of low-skilled workers. Comparisons based on the 1985/86, 1991, and 1997 LSMS suggest that incomes fell substantially for households with heads with different amounts of schooling.[14] Moreover, using data from

[14] Glewwe and Hall (1998) report results based on a panel of households *in Lima only* included in the 1985/86 LSMS and in a follow-up survey conducted in 1990 which suggest that households in which the head had more schooling suffered smaller income losses. I find no clear pattern: A simple tabulation of income by the education of the household head shows that mean income fell by more in households with heads with some secondary education (-41%) than in households with primary education or less (-27%), or

annual labor force surveys conducted in Lima, Saavedra (1998) and Saavedra and Maruyama (1998) show that the rate of return to education estimated from a standard Mincerian regression fell noticeably during the crisis (from .11 in 1987 to .08 in 1991), and rose steadily thereafter (to about .13 in 1995). It therefore seems unlikely that changes in access to credit or changes in the marginal benefit to education can explain the patterns in school attendance and completion rates I observe.

The effect of the crisis in Peru on education outcomes appears to have operated through a substantial reduction in forgone income. Real wages in urban areas dropped precipitously during the crisis. The results in Saavedra (1998) for Lima suggest that the mean real monthly wage for informal sector workers fell from about 600 soles in 1987 to 200 soles in 1991, before recovering partially to about 400 soles in 1987. The estimates for formal sector workers are even more staggering, indicating a drop from 1200 soles to 200 soles between 1987 and 1991, and a recovery to 800 soles by 1997. These results suggest that there was a massive decline in the opportunity cost of schooling in Lima during the crisis.

Children who hold jobs in Peru in "normal" years may have been less willing and able to combine school and work during the 1988-92 crisis. The extent to which a reduction in child employment frees up time for schooling or child leisure is a hotly contested topic in the literature on child labor (see, for example, Akabayashi and Psacharopoulos 1999; Basu 1999; Basu and Van 1998; Jensen and Skyt Nielsen 1997; Psacharopoulos 1997; Ravallion and Wodon 2000; Rosenzweig and Evenson 1977). The three Peru LSMS have no time allocation data, and can therefore not be used to answer this question conclusively. But the results I present in this paper are consistent with some substitution between employment and schooling: During the 1988-92 crisis, children in Peru were less likely to combine school with work, and they were more likely to make adequate grade progress. Arguably, children who were not working could expend more "effort" in school.

Some possible concerns with interpretation of my results are the effects of late school entry; possible changes in public expenditures on education; changes in the quality of education; and migration patterns, specifically migration from rural to urban areas. I discuss each of these in turn.

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some tertiary education (-25%). The recovery in incomes between 1991 and 1997, by contrast, clearly favored households with more schooling: The incomes of households with heads with primary education or less increased by 2% between 1991 and 1997, by 19% for households with some secondary education, and by 76% for households with some tertiary education. Note that long-term trends in the marginal benefits to schooling are unlikely to account for my results because education attainment *improved* between 1985/86 and 1991, and *worsened* between 1991 and 1997.

Late school entry may be a concern because of the way I define crisis exposure. There is some evidence from Ghana that parents delay schooling entry if their children are malnourished (Glewwe and Jacoby 1995). If the 1988-92 crisis in Peru led to a deterioration in nutritional status, then young children surveyed in 1997 whom I have defined as "unexposed" to the crisis (because they were not of school-age during the crisis) may have started school late, and therefore have completed fewer years of schooling for their age than they would have in the absence of the crisis.[15] There is no completely satisfactory way of resolving this problem, but I do not believe that this is the explanation for the patterns I observe. First, enrollment rates for young children are very high throughout, suggesting that late entry is not much of an issue in urban Peru. Second, in separate results (not reported, but available from the author upon request), I show that school attainment is higher in 1991 than in either 1985/86 or 1997. Of course, it is possible that the improvement in attainment between 1985/86 and 1991 is explained by an upward underlying time trend, while the deterioration in attainment between 1991 and 1997 is explained by patterns of late entry for young children exposed to the crisis, but this seems unlikely to me. Moreover, a decree which was passed between the 1995 and 1996 school years in Peru whereby all children who completed first grade were automatically promoted to second grade could *downward*-bias the estimates of the crisis on school attainment I report. By construction, all first-graders aged 8 or younger in 1995, who stood to benefit from the automatic promotion policy, are defined as unexposed to the crisis because they would be 10 or younger in 1997.

An increase in public expenditures on education during a crisis could reduce the marginal cost of education (for example, if the increase in expenditures takes the form of scholarships) or increase the marginal benefits (for example, if the increase in expenditures improves the quality of education). The Garcia government put no program in place to attempt to encourage schooling attainment during the crisis. Indeed, as Figure 6 shows, public expenditures on education followed roughly the same pattern as per capita GDP—rising sharply between 1985/86 and 1987, dropping equally sharply between 1987 and 1991, and steadily increasing thereafter. If anything, the constant attendance patterns and the improvements in age-specific grade completion during the crisis took place *in spite of* the evolution of public expenditures on education. Moreover, it is unlikely that households compensated for changes in public expenditures by increasing private expenditures on education between 1985/86 and 1991, and decreasing them thereafter: At best,

[15] I thank John Maluccio for this comment. In future research, I intend to look at the impact of the 1988-92 crisis on nutrition outcomes in Peru.

households may have been able to protect these expenditures somewhat in the context of dramatic cut-backs in overall household income.[16]

The fall in public expenditures on education during the crisis may have had an attendant effect on quality. This, in turn, could eventually affect the productivity and wages of cohorts of children who received their education in crisis years.[17] One way to test for these effects would be to supplement standard Mincerian wage regressions with measures of crisis exposure, as well as (possibly) with an interaction term between schooling and exposure. The 1988-82 crisis in Peru is too recent to assess these possible long-term consequences, as many of the affected cohorts are still in school. However, the possible negative effect of the crisis on school quality is a caveat to my conclusion that the 1988-92 contraction had a "positive" overall effect on human capital accumulation.

Finally, migration is a concern because the sample is limited to Lima, and the urban areas of the coast and sierra. In Peru, migration has traditionally been from rural to urban areas, although there appears to have been an important migration from urban to rural areas in the mid-1990's. This reverse migration was a result of the increase in security in the countryside which followed the lower levels of activity of Shining Path and MRTA after 1992, as well as of a deliberate policy designed to encourage resettlement of abandoned rural areas. (This policy included the creation of special government programs, such as PAR, the Programa de Apoyo al Repoblamiento.) Given lower mean incomes and lower adult education levels in rural areas, a reasonable assumption is that the underlying propensities to attend school and be on-track are lower—or at least no higher—among the migrants than the sedentary population. Another way to think about this is that the migrant populations are likely to have lower "ability" to attend school and make satisfactory grade progress. Rural-to-urban migration would tend to diminish ability in the sample in 1991 relative to 1985/86, while urban-to-rural migration could also diminish ability in 1991 relative to 1997. To the extent that this is the case, the estimates of the effect of the crisis I report would be downward-biased: In the absence of migration, the estimated coefficients on the measures of crisis exposure would have been even larger.

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[16] Data from countries affected by the 1998 East Asian crisis, for example, show a varied picture: In Indonesia, private expenditures on education fell both in absolute terms and as a share of total household expenditures, whereas in Korea expenditures on education fell by less than overall household spending (World Bank 2000, p. 121).

[17] I thank Francisco Ferreira for this point.

5. Conclusion

In this paper, I show that the profound economic crisis in Peru between 1988 and 1992 did not have a negative effect on the investments households made in the human capital of their children. Children exposed to the crisis were less likely to combine work with school, and had completed more years of education for their age than those unexposed to the crisis.

The call for a "socially responsible macroeconomic policy" which protects the poor during economic downturns has lately received a great deal of attention (this term, from Lustig 2000). There is no doubt that the poor often suffer disproportionately during crises—both in absolute terms, and because even a small reduction in their incomes can have dramatic effects on their welfare. The analysis in this paper shows, however, that the poor, like other households, are extremely reluctant to make cutbacks in key human capital investments. Indeed, because macroeconomic crises normally depress employment prospects, poor households may rationally choose to invest more, not less, in education.

Programs of targeted cash transfers conditional on behavior such as school attendance have been implemented in a number of Latin American countries, including the PROGRESA program in Mexico, and the Bolsa Escola in Brazil. These programs effectively lower the price of schooling for poor households and, insofar as the demand for schooling is reasonably price-elastic, may increase the schooling attainment of children in poor households. More recently, policy makers have also begun to consider increasing the budgets of these programs during a crisis. The aim of these crisis-induced budget increases would be twofold: To transfer additional income to poor households, and prevent school desertion. Transferring income to poor credit-constrained households during a crisis is likely to substantially increase their welfare. But the results in this paper suggest that the probability of dramatic, crisis-induced increases in school desertion may have been overstated.

Data Appendix

This Data Appendix discusses the reliability of sources of data other than the LSMS surveys which could potentially be used to evaluate the impact of the 1988-92 crises in Peru on education and employment outcomes. I also present details concerning the construction of variables used in the analysis.

Data reliability: I made several attempts to supplement the LSMS with other data, with only limited success. Demographic and Health Surveys (DHS) have been conducted in Peru in 1986, 1992, 1996, and 2000. Unfortunately, the 1986 survey did not collect information on the schooling and employment status of children. The DHS therefore have the disadvantage of not having data for the period before the crisis.

I use national accounts data for a general description of national economic trends in the 1980's and 1990's. Up to 1995, regional branches of INEI also computed estimates of GDP for each one of Peru's 25 departments. These data would have been useful for a difference-in-difference analysis of the impact of the crisis. But combining these data with the survey data raises serious problems of measurement error. The department-level pattern of crisis and recovery which comes out of the national accounts data is strikingly varied—indeed, so varied as to seem less-than-credible given anecdotal evidence about the depth of the crisis throughout the country.[18] There are also problems merging these data with the household-level data from the surveys. Only 15 departments were covered in both the 1985/86 and 1991 surveys, and the sample sizes vary wildly across departments—ranging from more than 2,000 individuals between ages 6 to 17 in Lima to less than 50 in some of the smaller departments. A further source of measurement error is introduced by the fact that the department-level GDP figures purport to cover both urban and rural areas, whereas the analysis conducted in this paper is based on the urban samples of Lima, the coast, and the sierra only. For these reasons, I do not provide difference-in-difference estimates of the impact of the crisis.

The Ministry of Education keeps information on total public recurrent and capital expenditures on education during the period, and I use these data to inspect whether changes in school outcomes could be attributed to changes in spending patterns, rather than to changes in the economic environment. These data are considered to be reasonably accurate and comparable

[18] This large regional variation in the estimates occurs even though INEI "imputed" part of the department-level flows using data for the entire country: For example, in some economic sectors, national changes are attributed to different departments according to the fraction of the stock of total employment or total industry output located in each department. In general, one would expect that this procedure would tend to make estimated department-level growth rates *more* similar to each other.

over time. I also attempted to use administrative data on enrollment and repetition levels, but discarded these as unreliable. Administrative data on enrollment in various grade levels is collected in Peru from the school headmasters themselves. This creates a serious incentive problem, as headmasters inflate enrollment figures to receive larger transfers. The last time that the Ministry of Education figures could be checked, by comparing them with the figures from the 1993 population census, there were estimated to be almost one million "ghost" students in the statistics supplied by the headmasters themselves. Moreover, these (highly unreliable) figures are available for only three years—1993, 1998, and 1999. The administrative enrollment and repetition figures for other years in the 1980's and 1990's are based on imputations which statisticians at the Ministry of Education themselves do using the LSMS surveys.

Construction of variables: In this paper, I consider the impact of the 1988-92 crisis on three outcomes: School attendance, employment, and the mean years of completed schooling. I define attendance based on two questions in the LSMS: All household members aged 6 or older are first asked whether they are "currently attending school or studying something", and those who answer "no" are then asked whether they "attended school or studied something in the last 12 months".[19] The variable I construct for attendance takes the value of one for all individuals who answer "yes" to either one of these questions. Note that it is important to take into account *both* these questions, as a large fraction of households in the 1985/86 LSMS were surveyed during the summer vacation months of December though March, so that attendance rates based on only the first question in this survey would appear to be unreasonably low. I also construct a variable for the total number of years of schooling completed based on questions about age and attainment in the surveys.

The measure of employment used in this paper is based on separate questions asked of all household members aged 6 or older. These questions first ask respondents whether they worked "as an employee for a business, corporation, government, a boss, or another individual", and second, whether they worked "for themselves" ("a cuenta propia", or self employed), "as an unpaid family member, or on the (family) farm". The reference period for both questions is the last week; follow-up questions with longer reference periods (12 months) are asked of those who

[19] This measure of "attendance" does not take into account the number of times children are actually going to school—a shortcoming of the data. Using the term "enrollment" would also be somewhat inaccurate, however, as children may be formally enrolled in school, but not attend. The Spanish word used in the questions in the surveys ("*asistir* al colegio") is closer to attendance than it is to enrollment (the translation for enrollment would be "estar *inscrito* en el colegio"). See Paxson and Schady 1999 for a similar construction of variables.

did not work in the last week. For the purposes of this paper, I consider all those who answer affirmatively to *any* of the four questions to have been employed.[20]

The Peruvian think-tank GRADE has calculated comparable consumption and income aggregates for the 1985/86, 1991, and 1997 LSMS, as well as for an LSMS conducted in 1994. Specifically, GRADE used 1997 price deflators to adjust for price differences across natural regions, and the national CPI to deflate consumption and income over time. I use GRADE's income aggregates for the estimations in this paper, but do not use the 1994 LSMS for the analysis because the GRADE aggregates suggest that mean income in this year was lower than in 1991, while the national accounts show a clear improvement in income per capita during this period. It is therefore unclear whether 1994 should be treated as a "crisis" year.[21] In addition to income, I take variables from the LSMS on household composition and size, age, and access to credit.

Table 4 provides details on the construction of the crisis exposure variable used for the OLS regressions in Table 3. Years of exposure is determined by the number of years a child was aged 6 to 17 during the 1988-92 period. For the age calculations, all children surveyed in 1985/86 are taken to have been surveyed in 1985, given that the majority of households were surveyed in 1985 rather than 1986.

[20] In the 1985/86 and 1991 surveys, working "on the farm" and working "for themselves, or as an unpaid family member" are asked separately, whereas in the 1997 survey both options are listed as part of the same question. Household chores are meant to be excluded from these categories, and are asked about in the following questions.

[21] See Deaton 2000 for an analysis and possible explanations of such divergences between the survey and national accounts figures for India in the 1990's.

Table 4: Exposure, by age and survey year

Age	Survey Year	Exposure
6	1985/86	0
7	1985/86	0
8	1985/86	0
9	1985/86	0
10	1985/86	0
11	1985/86	0
12	1985/86	0
13	1985/86	0
14	1985/86	0
15	1985/86	0
16	1985/86	0
17	1985/86	0
6	1991	1
7	1991	2
8	1991	3
9	1991	4
10	1991	5
11	1991	5
12	1991	5
13	1991	5
14	1991	5
15	1991	5
16	1991	5
17	1991	5
6	1997	0
7	1997	0
8	1997	0
9	1997	0
10	1997	0
11	1997	1
12	1997	2
13	1997	3
14	1997	4
15	1997	5
16	1997	5
17	1997	5

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Figure 1: Real per capita GDP and prices in Peru, 1980-1999

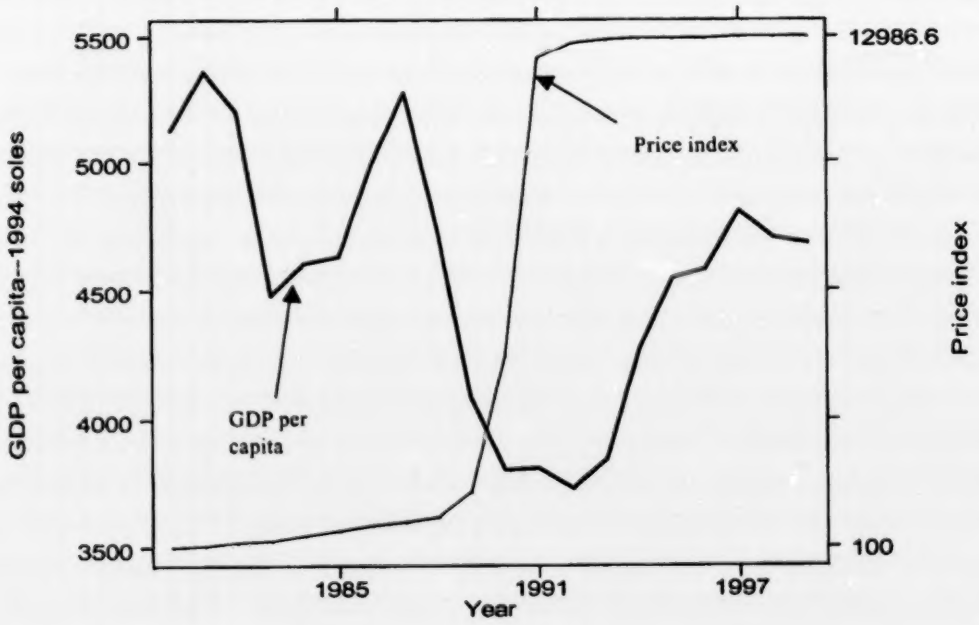


Figure 2: Age-specific school attendance rates, by survey year

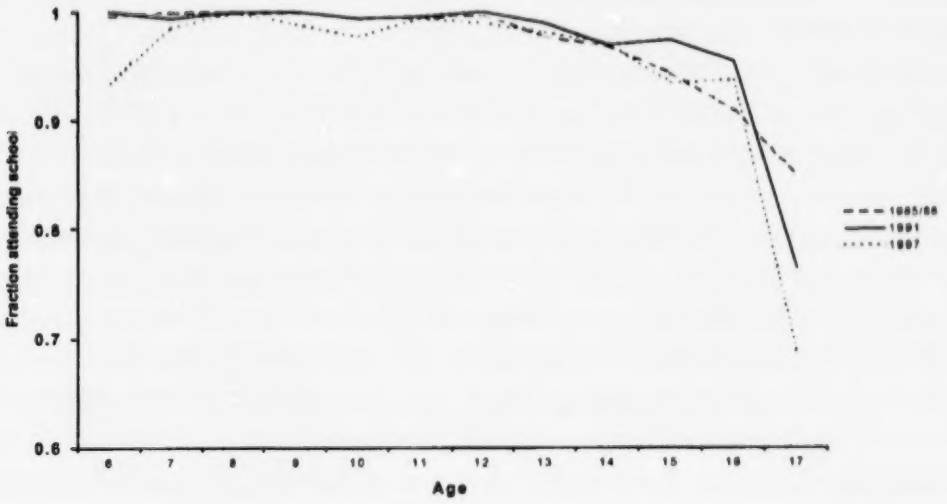


Figure 3: Age-specific employment rates, by survey year

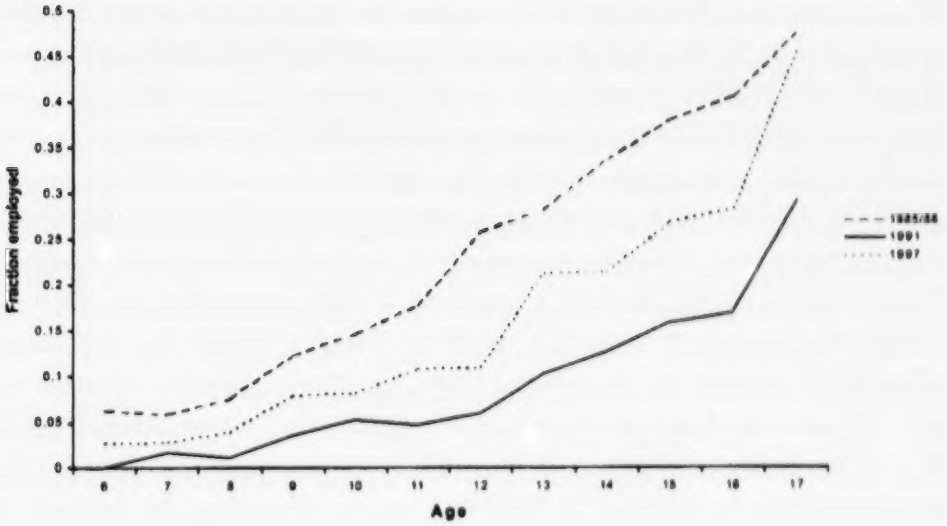


Figure 4: Years of Completed Schooling, by Age

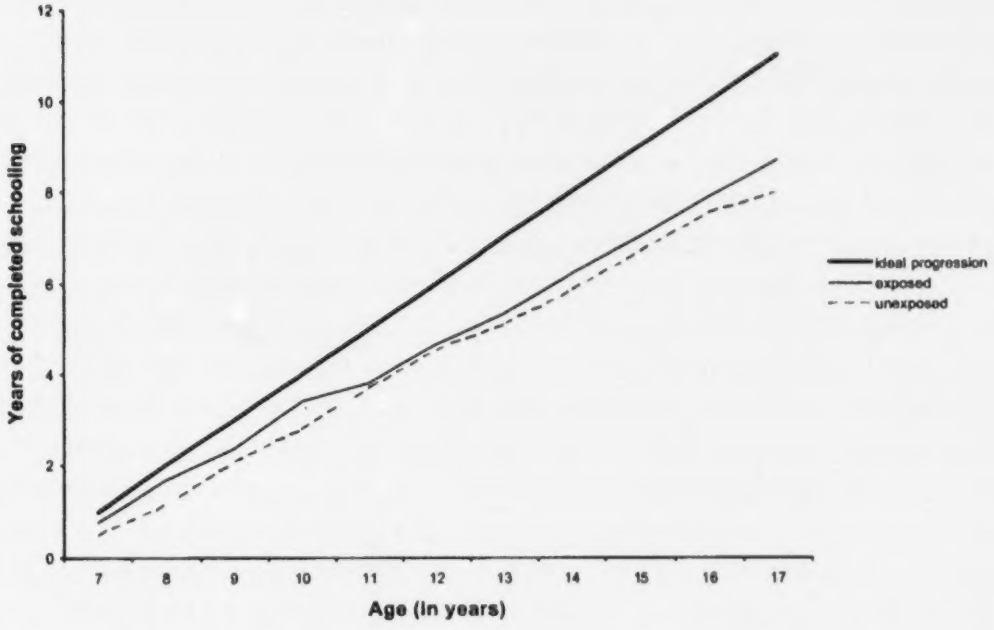
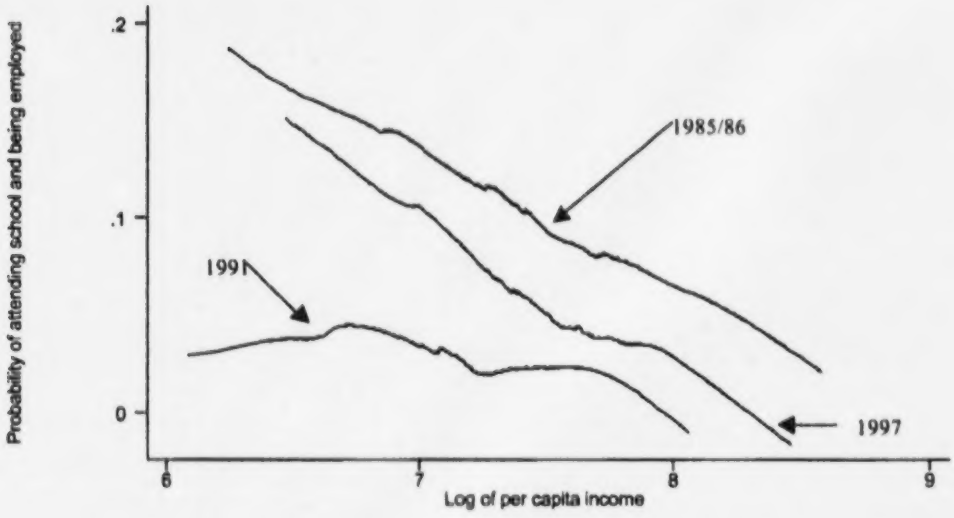


Figure 5: Probability of both attending school and being employed, by year

(i) 6 to 11 year-olds



12 to 17 year-olds

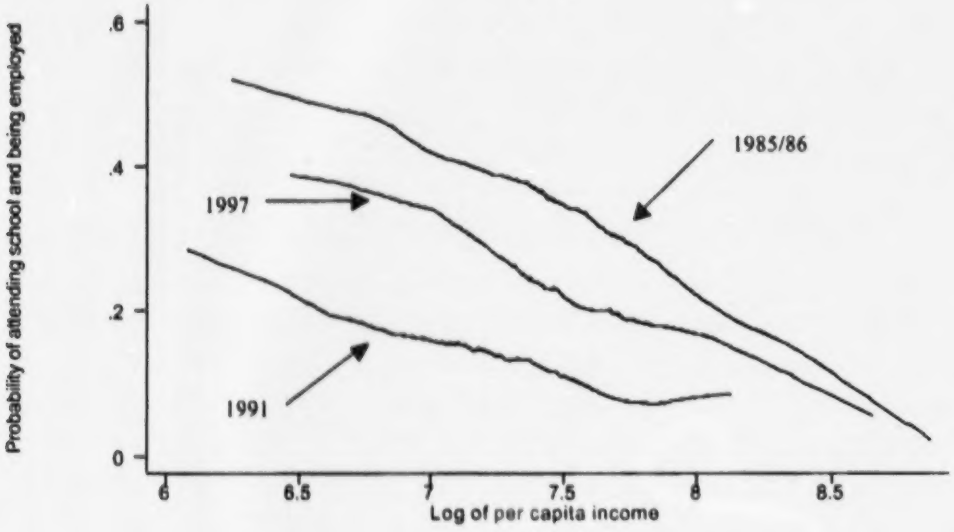


Figure 6: Recurrent and Capital Public Expenditures on Education, 1980-1997 (Constant 1997 soles)

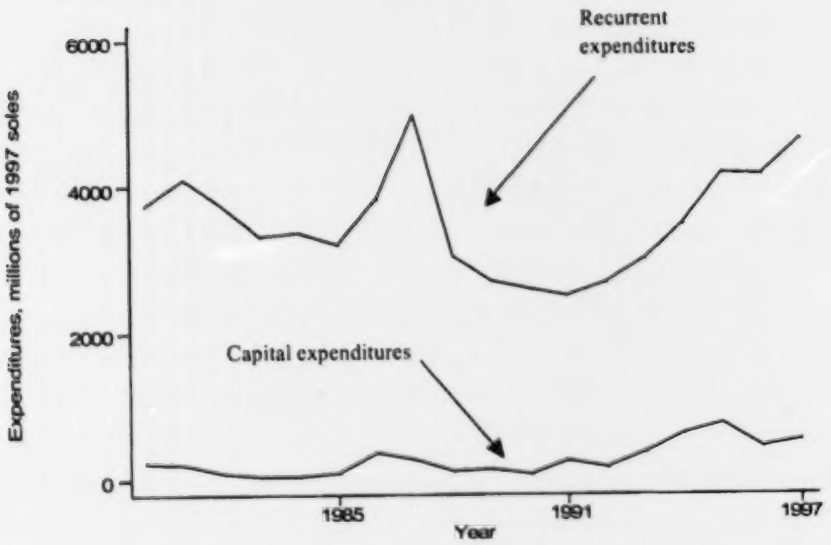


Table 1a: Sample means (unweighted), by region and year

	Urban sample (excluding urban selva)			Lima only			Urban Coast only			Urban sierra only			Rural sierra only		
YEAR	1985 /86	1991	1997	1985 /86	1991	1997	1985 /86	1991	1997	1985 /86	1991	1997	1985 /86	1991	1997
Prop. attending school only (6-11)	0.90	0.97	0.92	0.92	0.98	0.95	0.91	0.99	0.93	0.83	0.94	0.87	0.41	0.70	0.37
Prop. employed only (6-11)	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.01	0.00	0.01
Prop. neither in school nor employed (6-11)	0.00	0.00	0.02	0.00	0.00	0.02	0.00	0.00	0.02	0.00	0.00	0.02	0.01	0.00	0.01
Prop. both in school and employed (6-11)	0.10	0.03	0.06	0.07	0.02	0.04	0.09	0.01	0.05	0.17	0.06	0.11	0.57	0.29	0.60
Prop. attending school only (12-17)	0.64	0.82	0.71	0.68	0.83	0.74	0.62	0.87	0.71	0.56	0.75	0.65	0.15	0.36	0.17
Prop. employed only (12-17)	0.03	0.03	0.05	0.03	0.04	0.04	0.04	0.03	0.06	0.03	0.02	0.04	0.22	0.07	0.19
Prop. neither in school nor employed (12-17)	0.02	0.03	003	0.01	0.02	0.04	0.03	0.04	0.04	0.02	0.04	0.01	0.02	0.03	0.02
Prop. both in school and employed (12-17)	0.31	0.12	0.21	0.28	0.11	0.18	0.30	0.06	0.19	0.39	0.20	0.29	0.61	0.54	0.61
Proportion on-track for age (8-10) *	0.72	0.87	0.79	0.71	0.84	0.79	0.69	0.90	0.72	0.77	0.90	0.85	0.35	0.69	0.65
Log (per capita income)	7.64	7.34	7.64	7.82	7.44	7.77	7.42	7.27	7.45	7.48	7.22	7.58	7.22	6.92	6.63
Proportion with access to credit	0.22	0.16	0.40	0.23	0.15	0.39	0.18	0.15	0.40	0.23	0.21	0.40	0.10	0.11	0.19
Proportion rural	---	---	---	---	---	---	---	---	---	---	---	---	1.00	1.00	1.00
Mean age	25.27	26.71	28.92	25.98	27.27	29.20	24.35	26.91	29.10	24.83	25.60	28.08	24.42	25.60	25.20
Mean years of education of father	7.63	8.52	8.72	8.30	8.48	8.98	6.64	7.49	7.82	7.33	9.39	9.20	3.15	5.16	5.20
Mean years of education of mother	5.73	6.80	7.28	6.33	6.66	7.59	5.14	6.11	6.26	5.15	7.55	7.77	1.38	3.23	3.05
Mean household size	6.46	6.14	5.98	6.34	6.25	6.03	6.65	6.32	6.12	6.47	5.83	5.72	6.30	5.69	6.06

* Conditional on school attendance.

Table 1b: Sample means (unweighted), by year, gender, and income quintile (urban sample, excluding urban selva)

	Males			Females			Poorest income quintile			Richest income quintile		
YEAR	1985 /86	1991	1997	1985 /86	1991	1997	1985 /86	1991	1997	1985 /86	1991	1997
Prop. attending school only (6-11)	0.90	0.97	0.92	0.90	0.96	0.92	0.90	0.96	0.91	0.92	0.97	0.95
Prop. employed only (6-11)	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Prop. neither in school nor employed (6-11)	0.00	0.00	0.01	0.00	0.00	0.02	0.00	0.00	0.04	0.01	0.01	0.01
Prop. both in school and employed (6-11)	0.10	0.02	0.06	0.10	0.03	0.05	0.10	0.04	0.06	0.07	0.02	0.05
Prop. attending school only (12-17)	0.58	0.82	0.69	0.69	0.81	0.73	0.60	0.82	0.67	0.75	0.82	0.77
Prop. employed only (12-17)	0.03	0.03	0.05	0.04	0.03	0.04	0.05	0.03	0.05	0.02	0.03	0.02
Prop. neither in school nor employed (12-17)	0.01	0.03	0.03	0.03	0.03	0.04	0.04	0.03	0.04	0.02	0.02	0.04
Prop. both in school and employed (12-17)	0.37	0.12	0.23	0.25	0.12	0.19	0.32	0.12	0.24	0.21	0.13	0.16
Proportion on-track for age (8-10) •	0.74	0.87	0.77	0.70	0.88	0.80	0.60	0.88	0.74	0.81	0.91	0.87
Log (per capita income)	7.65	7.35	7.64	7.62	7.33	7.64	6.34	6.27	6.60	8.87	8.42	8.75
Proportion with access to credit	0.21	0.16	0.40	0.22	0.16	0.39	0.11	0.18	0.35	0.32	0.21	0.41
Proportion rural	---	---	---	---	---	---	---	---	---	---	---	---
Mean age	24.78	26.19	28.48	25.74	27.20	29.34	22.60	24.84	26.23	29.19	29.81	32.47
Mean years of education of father	7.61	8.42	8.70	7.65	8.62	8.73	5.52	7.13	7.07	9.67	10.35	11.20
Mean years of education of mother	5.68	6.79	7.26	5.79	6.80	7.30	3.81	5.66	5.60	7.87	8.93	9.97
Mean household size	6.46	6.17	6.02	6.45	6.11	5.94	7.26	6.55	6.59	5.26	5.10	4.84

• Conditional on school attendance.

Table 2: Odds-ratios of both attending school and being employed

	(i)	(ii)	(iii)
	Children aged 6 to 11		
1985/86	4.43***	4.11***	3.35***
	[1.23]	[1.34]	[1.22]
1991	--	--	--
1997	2.42***	3.15***	1.98*
	[0.72]	[1.16]	[0.78]
Pseudo R-squared	0.09	0.12	0.15
N	4,220	3,086	2,193
	Children aged 12 to 17		
1985/86	3.52***	2.68***	2.79***
	[0.52]	[0.44]	[0.51]
1991	--	--	--
1997	2.09***	2.39***	2.21***
	[0.32]	[0.42]	[0.47]
Pseudo R-squared	0.07	0.11	0.14
N	4,113	2,939	2,135

Note: The dependent variable takes on a value of one if children are both attending school and employed, and zero if they are in school only. Children are considered to be employed if they responded affirmatively to any one of the following four questions in the surveys: (i) "In the last 7 days, did you work as an employee for a business, corporation, government, a boss, or another individual?"; (ii) "and in the last 12 months?"; (iii) In the last 7 days, did you work for yourself, ("a cuenta propia", or self employed), or as an unpaid family member, or on the farm?"; (iv) and in the last 12 months?".

* Significant at the 10% level or better; ** significant at the 5% level or better; ***significant at the 1% level or better.

Standard errors in square brackets are corrected for heteroskedasticity and clustering.

Specification (i) includes strata and gender controls, and a vector of age dummies; specification (ii) supplements specification (i) with variables for the education of both parents (separately), variables for the number of household members under 3, between 3 and 5, between 6 and 8, between 9 and 11, between 12 and 14, and between 15 and 17, as well as a variable for the size of household; the fixed-effects specification in (iii) limits the sample to households living in districts which were included in the sample in 1985/86, 1991, and 1997, and includes the variables in specification (ii), as well as a dummy variable for each district.

Table 3: Average Number of Years Passed, Aged 6 to 17

	(i)	(ii)	(iii)
Years of crisis exposure	0.050***	0.044**	0.043**
	[0.02]	[0.02]	[0.02]
R-squared	0.81	0.86	0.87
N	8,646	6,203	4,917
Crisis exposure = 0	---	---	---
Crisis exposure = 1	0.008	0.057	0.035
	[0.07]	[0.08]	[0.09]
Crisis exposure = 2	-0.011	0.052	0.089
	[0.08]	[0.09]	[0.10]
Crisis exposure = 3	0.204**	0.165*	0.173*
	[0.08]	[0.09]	[0.10]
Crisis exposure = 4	0.184**	0.236**	0.234**
	[0.09]	[0.10]	[0.11]
Crisis exposure = 5	0.224**	0.204*	0.199*
	[0.09]	[0.10]	[0.12]
R-squared	0.82	0.86	0.87
N	8,646	6,203	4,917

Note: The dependent variable is the number of years of schooling completed.
* Significant at the 10% level or better; ** significant at the 5% level or better; ***significant at the 1% level or better.
Standard errors in square brackets are corrected for heteroskedasticity and clustering.
Specification (i) includes year dummies for 1985/86 and 1997, strata and gender controls, and a vector of age dummies; specification (ii) supplements specification (i) with variables for the education of both parents (separately), variables for the number of household members under 3, between 3 and 5, between 6 and 8, between 9 and 11, between 12 and 14, and between 15 and 17, as well as a variable for the size of household; the fixed-effects specification in (iii) limits the sample to households living in districts which were included in the sample in 1985/86, 1991, and 1997, and includes the variables in specification (ii), as well as a dummy variable for each district.

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